## Koch, Kristine

**From:** Koch, Kristine
**Sent:** Monday, October 27, 2014 9:12 AM
**To:** PETERSON Jenn L
**Cc:** GAINER Tom
**Subject:** RE: Portland Harbor Food Web Model Issues

Thanks Jenn. We share some of your same concerns. I hope we'll finally be able to resolve these issues and move forward.

See you Thursday.

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**From:** PETERSON Jenn L [mailto:PETERSON.Jenn@deq.state.or.us]
**Sent:** Monday, October 27, 2014 8:19 AM
**To:** Koch, Kristine
**Cc:** GAINER Tom
**Subject:** Portland Harbor Food Web Model Issues

Hi Kristine,

I apologize for not getting you my food web model issues sooner. I am attaching a subset of my comments from 2008 that I think summarize my biggest issues. I am looking forward to discussing this Thursday.

Jennifer

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**From:** PETERSON Jenn L
**Sent:** Tuesday, June 17, 2008 3:07 PM
**To:** 'Blischke.Eric@epamail.epa.gov'; 'Shephard.Burt@epamail.epa.gov'; Humphrey.Chip@epamail.epa.gov
**Cc:** Goulet.Joe@epamail.epa.gov; jeremy_buck@fws.gov
**Subject:** Food Web Model Comments
**Attachments:** Portland Harbor FWM; BruceHComments R2 App H.doc; PH FW model Bruce H comments.htm; AppendixEComments061208_JPComments.doc

Eric and Burt,

I have added my comments to Eric's, which was a good start to the comments. I know we have been busy with other things, but this model really needs to be run correctly to meet project objectives. I am concerned, because it doesn't appear that the meetings of a year or so ago resulted in an agreed upon product. The model is over calibrated, and the uncertainty and sensitivity analysis so limited they seem useless. Despite schedule demands, I hope we can given this the attention it needs to produce a good tool for decision making at the site. I hope we can discuss before the comments go to LWG. I am also attaching Bruce's comments - I am sure you got these but they didn't make it into the comments that were pulled together so I thought I would include just in case. I would also like to fax you a summary from Gobas when you get back in the office. Again, here is a re-cap of my biggest issues:

1. Uncalibrate the model. Run the model forward to evaluate observed versus predicted values in fish tissue for all samples across the harbor (not as an average).

2. Focus on congeners, and revisit the list used for modeling. The congeners they have selected do not represent the ones that represent the most risk or in some cases doesn't even represent ones that were detected in fish tissue with the most frequency. We need to understand the implications of modeling mixtures, but more importantly we have to know that the model works for individual chemicals, because that is the only "real" data showing sediment, water and tissue distributions. This should focus on at least some TEQ congeners.

3. Refine how water is used in the model. We need to get this right because it is a very sensitive parameter for the model, and sediment / water contributions will likely be a topic of debate in the project. Organic carbon is an important partitioning phase, but the way the water data available at the site should be used in the food web model has not been resolved. Our dissolved filter was 0.5 um - quite a bit larger what would be used for truly dissolved or bioavailable (0.2 um). Dissolved organic carbon is considered to be comprised of particles smaller than 0.45 um diameter. We made a comment a few iterations ago that the empirical data should be considered in model development / calibration, and they responded by removing total water values from the model entirely, and no overlying water was used. This issue needs further discussion, but the result is that the water data used for the model was limited to only water stations that collected filtered XAD values. Only 3 water transects were used in the model (integrated). This is also not consistent with the most recent model by Gobas (2004). The result of this change, as stated by the LWG, is that the bioavailable concentration in water is reduced by 1/3 (see e-mail from Nancy to Bruce H). Also, other bioavailable terms in the model are no longer used (e.g. POC). I am not sure why the use of empirical data would modification using other equation (one that doesn't even match the original citation of Morrison 1997 (see page 5 of attachment E1), but we may need to consider returning to the original equations.

4. Move away from using standard errors on mean data as distributions. The focus on the mean misses the whole point of including uncertainty / sensitivity analysis does not give us the information we need on variability and uncertainty in the empirical data and resulting model predictions. The approach used here is really no better than selecting point estimates for each parameter.

**Additional Comments:**

Temperature: They used a mean temperature of 13.6 C, and only varied this parameter relative to the standard error on the mean. The full distribution of temperature should be used (see comment above). This is a sensitive parameter, so we need to have a path forward. If we want to move away from distributions to describe the sensitivity of this parameter we should pick an upper confidence, as we did for the fish dietary approach.

TSS: All data should be used, not just near bottom.

Dissolved organic carbon and Water chemistry: Only the standard error on the mean was used in the sensitivity analysis. Distributions of all the data should be used, or we should settle on an appropriate deterministic value that is an upper confidence.

Sediment Data: Move away from input parameters of SWAC values for organic carbon and concentration. Present as a distribution. Do not use Thiessen polygons to estimate sediment TOC and chemicals concentrations in the surface sediment. Use distributions of the empirical data. Include distributions of sediment in calibration. The LWG states "because the primary purpose of model development for this report was generation of iPRGs, the uncertainty surrounding estimates of sediment concentration was not a primary concern of model calibration."